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EDITORIAL.

PROBABLY at no time since anæsthetics were first used have there been so many different agents from which the administrator can choose as there are to-day.

Gases, volatile liquids, and those less volatile, are at his disposal in large numbers, and the methods by which they may be employed are also numerous and diverse. The results of these novel activities are in some respects no doubt admirable. Going under an anæsthetic, for example, is—at any rate in expert hands—by no means the unpleasant or terrifying process which it was almost inevitably until recent times. Nor are subsequent discomforts as usual, or as lasting, or as formidable as they were wont to be. At the operation itself the surgeons' convenience can be assured to a high degree, and this also contributes greatly to the patients' welfare. Nevertheless, no thoughtful anæsthetist is satisfied with the position to which he has attained. One consideration alone, if there were no others, would prevent him from any excessive complacency. That consideration is the fatality rate of anæsthesia. There is no evidence to suggest

that this is diminishing, still less to prove anything of the sort. On the contrary, such evidence as can be obtained on a matter which is extraordinarily difficult to estimate, suggests that if anything the rate is rising. Anæsthetists cannot and will not be content for the results to rest there. No doubt deaths on the operating table are, in expert hands, so rare as to be almost negligible. The same cannot be truthfully claimed for results following operation. Until we can assert with confidence that not only will the patient leave the table in safety but that also, as far as the anæsthetic is concerned, his subsequent progress to health will be uninterrupted, not until then should we be satisfied with our drugs and with our methods.

THE PRIZE ESSAY.

Judgment of the various essays sent in to compete for the *Journal's* prize is still in progress. We hope to be able to announce the result in our next issue, and in that or the following number to publish one or more of the essays which have been sent in for judgment. The process of elimination is necessarily slow because those who are deciding live far apart.

SPINAL ANÆSTHESIA

By Professor Sebrechts, Bruges.

AS regards anaæsthesia no surgeon will deny that it is a factor of paramount importance for the success of surgical operations. Not only does the modern surgeon select an anæsthetic agent which is as innocuous as possible for the organism, and which enables him to operate without causing pain, but he exercises discrimination and selects for each patient and for each operation the method of anæsthesia which will permit the best surgical technique to be employed. The surgeon wishes to obtain as perfect a field as possible for the intended operation: asepsis, muscular relaxation, the volume of the abdominal organs, the bleeding of the tissues, etc., are greatly influenced by the method of anæsthesia.

Briefly, if the anæsthesia must render the operation painless and without danger to the patient, it must also enable the surgeon to perform the operation with ease, because an easy operation is as a rule better carried out and because the more perfect the technique the better are the chances of recovery. If we take all these factors into consideration, we must confess that we do not possess at the present time an ideal method of anæsthesia for all purposes. Such an anæsthesia should during a fairly long time:

(1) suppress the anguish of the patient and abolish sensation entirely,

(2) afford to the surgeon perfect muscular relaxation, a maximal retraction of the internal organs and a bloodless field of operation,

(3) not intoxicate the patient, and not injure his vital functions or cause post-operative complications.

I am convinced that scientific progress will one day make

such anæsthesia available to the surgeon, but meanwhile we must be content with selecting for each patient and for each operation the kind of anæsthesia which is nearest to the ideal.

In my wards we practise every method of anæsthesia, taking into account the above-mentioned factors, but I sincerely believe that for most cases of abdominal surgery it is spinal anæsthesia (under which I have performed with my students about 35,000 operations) which offers the greatest advantages.

Whatever the method of anæsthesia and whatever the anæsthesia substance the surgeon selects, the individual sensibility of the patient must always be taken into account. It is a mistake to calculate the dose of an anæsthetic agent exclusively according to the weight of the patient. A given dose of chloroform will cause deep anæsthesia in one subject, and be insufficient with another, although both are of the same weight. There thus exists an individual susceptibility and sensibility. I will even go further and say that the subject's sensitivity may vary in the course of his life under certain physiological (e.g. pregnancy) as well as pathological conditions such as are caused by intoxication: Through the bile: stenosis of the choledochus; through his urea: prostatic patients; through the products of intestinal retention: in acute obstruction; through the products of cellular necrobiosis: cancer of the ovary, etc.

All these physiological or pathological conditions increase the sensitivity to anæsthetic agents. The individual sensitivity is at the same time familial: generally speaking, it can be said that all the children of the same family resemble each other and have the same tolerance for anæsthetic agents, unless it is temporarily or permanently modified by one of the above-mentioned physiological or pathological conditions.

Furthermore, this sensitivity to anæsthetic agents varies according to the race. The Anglo-Saxon, for instance, has resistance to larger doses than the Italian, etc.

The conception of individual and familial sensitivity of constitutional origin, a sensitivity which is constant with sub-

jects in normal conditions, but variable in certain physiological circumstances, has been denied by certain of our teachers who are not clinicians, but pharmacologists. It is certain, however, that even animals exhibit differences of sensitivity to anæsthetic agents, though not to the same extent as man. The above opinion, which we have held for more than 15 years, is now an acknowledged fact, and has been confirmed by numerous authors.

The principle of individual sensitivity is indisputable for the surgeon who practises spinal anæsthesia. We noticed long ago that a normal dose of the anæsthetic agent causes too deep an anæsthesia, or even alarming symptoms, with certain subjects, whom we called "rachi-sensitive", whereas with others, whom we called "rachi-resistant", the same dose causes an insufficient anæsthesia or no anæsthesia at all.

It is by chance that I discovered the curious fact of the familial nature of that abnormal sensitivity to spinal anæsthesia, having had the good fortune of operating upon five members of the same family, the father and his four sons, every one of whom proved refractory to spinal anæsthesia. Had I not had the opportunity of meeting with so complete a type of rachi-resistance (such a type being very rare), and if I had not had the chance of operating on five members of the same family, I would not have discovered that fundamental fact, and might perhaps have abandoned this method of anæsthesia as being unreliable, as so many others have done.

Besides abnormally resistant individuals and families, there also exist individuals and families that are abnormally sensitive to spinal anæsthesia. We know families, several members of which are sensitive to spinal anæsthesia to such an extent that it is necessary to reduce appreciably the normal doses to avoid an accident; fortunately these cases are very rare.

It has been proved that certain subjects are hyper-sensitive to spinal anæsthesia, others hyper-resistant; as, however, it is not yet possible to determine with certainty before the anæsthesia the degree of sensitivity of a given patient, it is necessary to adopt a technique which permits

beginning the spinal anæsthesia with a small dose which is devoid of danger, even for a rachi-sensitive subject, and to increase gradually that dose until the optimum degree of anæsthesia for a given patient and a given operation is obtained. In other words, it is essential to be able to realize the "individual dosage" for spinal anæsthesia, just as for any other method.

It is because this fundamental question has not been recognized and because the individual dosage has not been realized that, in spite of its indisputable advantages, spinal anæsthesia has been abandoned by prominent surgeons. Fear of an insufficient anæsthesia, and more so of a serious accident, without mentioning the fear of post-operative sequelæ, deprives the surgeon of the required confidence, and he prefers to abandon the method of anæsthesia which has caused him trouble, worry, and, possibly, accidents.

Spinal anæsthesia has always deserved the objection of being dangerous on account of the difficulty of realizing the individual dosage: the total dose was injected at once without the surgeon knowing whether it would be insufficient, sufficient or excessive. Of course, the experienced rachi-anæsthetist can roughly appreciate the resistance of a given patient and if necessary re-inject a second dose, or complete an insufficient anæsthesia with an injection of morphine-scopolamine or a chloroform inhalation; in the same way as he can diminish a too deep anæsthesia with ephedrine or even adrenalin intravenously and other efficient measures.

But for the majority of surgeons who have not had much experience of it, spinal anæsthesia when performed according to the usual technique (specially when operating upon the upper abdomen) is justly considered as a very unreliable procedure, and only to be resorted to as a last resource in cases when the other methods of anæsthesia are impossible or contraindicated.

I have never tired of drawing the attention of colleagues who honoured me with their visit, to the very real dangers of spinal anæsthesia and to the means of avoiding them.

Being conscious of the responsibility which rests with an author who publishes a method which can be dangerous when used by others, I abstained until 1928 from publishing

the technique which I was then following. This technique had rendered us the greatest service, but it certainly required a great deal of practice. Since then I published in the *Bulletin de l' Académi de Médecine* a note, in which I condensed all the interesting facts which I had observed. I endeavoured to explain these facts by elementary theories of anatomy and physiology resulting from my investigations.

Derom, in his experiments with about 60 dogs, has been able to confirm my hypothesis this year. This author, whose work has been rewarded with the Alvarenga prize, has brought an important contribution to the investigations on the physio-pathological basis of spinal anæsthesia.

As a result of the work of the clinicians and the experimental work carried out in laboratories, spinal anæsthesia, which in its initial stage was a purely empiric method, has gradually become a method of anæsthesia, the action of which is fairly well known and which will be further improved.

During the last few years we have worked out a method which gives us entire satisfaction and which can be used by any assistant, subject to his being able to make a clean and painless spinal puncture.

This method, which is a modification of that of Howard Jones, is based on the use of a very dilute solution of percaine, which is injected in a progressive manner.

(1) Percaine, a quinoline derivative, possessing a potent anæsthetic power, is remarkable first of all on account of the duration of its action. It is the only known substance which allows in all cases a muscular relaxation of sufficient duration to permit performing without hurrying the longest operation.

(2) The injection, without preliminary withdrawal of spinal fluid, of a large quantity of a very dilute anæsthetic solution which is lighter (hypobaric) than the cerebro-spinal fluid, has been introduced by Howard Jones (London), who demonstrated that the subarachnoid space is not a sack but a tissue made of very loose meshes, and which must be infiltrated with a very weak solution, as is done for the local anæsthesia of a tissue.

(3) We have shown that it is possible to make the spinal puncture, change the position of the patient, and to leave the needle *in situ* during half an hour without running the slightest risk of causing a lesion of the medulla or of the nerve roots, thus allowing the progressive injection of the anæsthetic solution until the optimum effect for a given subject and a given operation is obtained.

Our technique is very simple. As a rule we give the patient one hour before the anæsthetic, a subcutaneous injection of 1 c.c. sedol (morphine 0.01 grm., scopolamine 0.0004 grm). Aged patients, intoxicated subjects and the bad risks receive no sedol, but sometimes stimulants. This applies to urgent cases, too, because it is hardly possible in emergency cases to wait an hour before beginning the anæsthesia.

We consider the interval of one hour indispensable for the following reason. When the patient has received the sedol injection, his nervous system may fight against the action of the narcotic agent for a more or less prolonged period. After one hour, however, the narcotic agent has developed its maximum effect. When the patient is taken to the operating theatre one hour after the sedol injection, it is possible to foresee to a certain extent what his resistance to the spinal anæsthesia will be; a patient who is excessively rachi-sensitive will sleep deeply and consequently will require a very small dose of percaine.

The patient who is excessively rachi-resistant will exhibit no somnolence; he will be very much frightened or will assume a bold attitude, and will be entirely awake. Such a patient will require a large dose of percaine.

The moderately sensitive patient (whom me may call "normal", since this type represents the majority) will be sufficiently awake to take an interest in what is going on, but sedol will make him remarkably resigned and confident.

He will require a moderate dose, i.e. 15 to 20 c.c. of percaine solution 1:1500 injected according to our slow and progressive method (if the injection is made more quickly a smaller dose will be required). The preliminary sedol injection serves to a certain extent as a test of the degree of sensitivity of the individual to spinal anæsthesia.

The patient is then placed in a sitting position or in the lateral decubitus with head and back well bent towards the knees. The skin is disinfected with tincture of iodine. One raises between L_3 and L_4 an intradermal wheel with a very fine needle using a 1 per cent novocaine (or percaine 2:1000) solution with adrenalin, whereupon the needle is pushed forward into the depth of the tissue, and 1 to 2 c.c. of the above solution are gradually injected; this serves at the same time to determine in advance the direction the anæsthetist will have to give to the spinal needle, for it to penetrate without "feeling about" between the spinous process. The tissues of the region being anæsthetised, the spinal puncture is then made with a very thin nickel needle without stylet, or from which the stylet has been removed after the puncture of the skin.

The spinal needle is very gently pushed until the first drop of cerebro-spinal fluid appears, whereupon the needle is advanced about 2 mm. deeper. The whole procedure is entirely painless. If the puncture has been made in a sitting position, which is more convenient with patients who are obese, or whose spine is deformed or stiff, the position of the patient is now changed.

Howard Jones's percaine solution 1:1500 being lighter than the cerebro-spinal fluid *must not be injected in a sitting position*; it must be injected in such a position as will soak first of all the posterior sensitive roots which supply the nerves to the region of the proposed operation.

For a strictly unilateral operation (on an inferior limb, hernia, appendicectomy, kidney) the patient is placed in the lateral decubitus *on the opposite side*. For operations which are not strictly unilateral the patient is placed *in ventral decubitus*. *If the change of position is carried out with caution there is no danger of injuring the medulla or the nerve roots*, and the cerebro-spinal fluid issuing from the needle is never mixed with blood.

If one uses a fine nickel or rustless metal needle, to the exclusion of steel needles, there is no danger of breakage.

The patient being in the lateral or ventral decubitus, if the intended operation is on the upper abdomen, the operating table is left in the horizontal position. If the operation is on

the lower abdomen, the head of the table is lowered so as to allow the patient to be in the slight Trendelenburg position.

At this stage one makes sure once more that the needle is in the correct position, by observing the cerebro-spinal fluid issuing from its extremity, and then one injects a dose of Jones's solution 1 :1500, which even for a subject who is excessively rachi-sensitive will cause no accident, i.e. 5 c.c.

The stylet is again pushed into the needle, or if a needle without stylet has been used, the syringe is left attached to the needle. The operator then waits, carefully watching the pulse of the patient. If it slows down, attention is necessary. A slowing down of the pulse, even if pronounced, may be quite temporary: with a frightened or weakened patient this slowing down announces the ordinary syncope, which has nothing to do with spinal anæsthesia, but generally the slowing down of the pulse shows that the anæsthetic agent has reached beyond the sixth thoracic segment and has soaked the orthosympathetic fibres which accelerate the heart and accompany the anterior motor roots.

If this impregnation of the orthosympathetic heart accelerator is caused in the horizontal, or *a fortiori*, in slight Trendelenburg position by a still very diluted anæsthetic agent—for instance, by the first 5 c.c. of percaine solution 1 : 1500 injected between L_3 and L_4—it shows that the patient is extremely rachi-sensitive. The immediate slowing down of the pulse indicates the necessity for waiting five minutes, whereupon one ascertains by pinching the skin in the region of the operation whether the anæsthesia is sufficient for the operation.

If after waiting five minutes the pulse maintains its frequency, or if the slowing down disappears, or if the anæsthesia does not set in, one can inject another 5 c.c. of solution and re-inject 5 c.c. at intervals of five minutes until the patient no longer feels the pinching of the skin in the region of the operation. It is absolutely useless and very detrimental to the patient to ask him whether he feels the pinching of the skin. It is sufficient to watch his face.

The slightest twitching of the eyes must be considered as a proof of insufficient anæsthesia. I do not consider it necessary to take the blood pressure of the patient; a moderate

hypotension is a constant and even favourable phenomenon, as it diminishes the capillary oozing. A pronounced hypotension is easily perceived from the pulse of the patient. If it occurs suddenly during the course of the anæsthesia *according to the technique which we recommend*, one should not take alarm; it is the sign of a non-serious syncopal tendency caused by fear; more frequently it is preceded by a feeling of nausea; in order to make it disappear, it is sufficient to reassure the patient, bathe his face with cold water, get him to respire deeply, and, if necessary, to administer a little ether or oxygen.

We do not advise the injection of ephedrine as a preventive measure, with the exception of patients who are particularly frail, because it diminishes the quality of the anæsthesia and increases the blood-oozing.

On the other hand we are in favour of using ephedrine, and preferably adrenalin, when in the course of the anæsthesia, as a consequence of human imperfection (too rapid injection, too sudden movements of the patient), really alarming symptoms take place.

When the anæsthesia appears to be sufficient, one must wait another five minutes before changing the position of the patient. He is then placed on the operating table in the slight Trendelenburg position, care being taken to avoid any quick movement, and he is put in as comfortable a position as possible. It must be borne in mind that whereas hypotension facilitates the surgeon's work, it makes it necessary to take special care of the patient's comfort. For instance, in order to avoid the decubitus at the level of the heels and of the sacrum resulting from the pressure exerted by the weight of the body on the soft tissues in which the circulation is inhibited, it is indispensable to fit the operating table with thick rubber cushions or air matresses. Even the bed of the patient must be sufficiently soft to avoid pressure of the soft parts on the osseous edges (heels). It is also necessary to protect the radial nerve from pressure on the table. After having prepared the field of operation, the operation may now be commenced, i.e. about 35 minutes after the beginning of the anæsthesia. This interval may seem very

long, but as a matter of fact the facility afforded by this method to the technique of the operation enables one to regain rapidly the few minutes devoted to the anæsthesia; furthermore, the duration of the anæsthesia is such that the busy surgeon can very well anæshetise two or more patients simultaneously.

In any case, when using percaine, I must emphasise the absolute necessity of not shortening this period of waiting before starting an important operation, specially when operating on the upper abdomen; this applies even when anæsthesia sets in very quickly. Percaine from the point of view of the manifestations caused by spinal anæsthesia is a "delayed" anæsthetic agent, and when I started using the product, I noticed several times that the maximal depression of the orthosympathetic (characterized by hypotension and bradycardia) as well as paralysis of the intercostal and phrenic nerves (characterized by apnea) only manifested themselves after 25 to 30 minutes.

The anæsthetic "storm" is thus much more retarded with percaine than with the other products. If, for instance, one begins an operation on the stomach too soon, and specially if one neglects to anæsthetise the pneumogastric nerves at the level of the cardia after having opened the abdomen, somewhat pronounced tractions on the stomach are liable to cause very unpleasant and sometimes even dangerous reflexes, which may compel the surgeon to interrupt the operation.

If, on the contrary, one waits long enough before commencing the operation, one can perform the latter without any untoward incident, irrespective of its duration.

As regards the dose percaine to be injected, it may vary for the same operation. For most patients it averages from 15 to 20 c.c. of the 1 : 15000 solution injected slowly according to our technique.

As a result of this technique, which we have applied in a considerable number of cases, spinal anæsthesia, of which nobody denies the advantages, has lost the factor of uncertainty attached to it in the past, *and it is now possible to obtain in all cases a perfect anæsthesia without danger.*

Does this mean that no more accidents or untoward incidents will take place? The imperfection of human nature is such that nobody would answer the question affirmatively. Surgery is practised by men who, from time to time, make a mistake which could have been avoided, but one will no no longer witness failures and disasters which were unavoidable because it was impossible to foresee them, and which discouraged a large number of enthusiastic adepts in spinal anæsthesia. Both insufficient and too deep anæsthesia can unquestionably be avoided.

What about cephalalgia and paralysis and all the painful post-operative sequelæ which have been described? All these no longer exist.

It is true that a latent medullary affection can be revived or can continue its evolution after a spinal puncture, and I believe that it is preferable not to use spinal anæsthesia in such cases, but apart from pre-existing medullary affections, I believe that paraplegia is always the result of a faulty technique; e.g., if the anæsthetic solution has been injected into the medulla, thus dissociating the nervous tissues, or if the medulla has been repeatedly punctured, which causes a section of the fibres and hæmatomyelia, or if the vessels have been injured with consequent hætorachis. The puncture of the medulla, just the same as that of the brain, causes no pain; pain only manifests itself when the needle touches a posterior root.

Formerly the routine consisted in performing the spinal puncture sometimes on highly-strung patients, without previous anæsthesia of the skin, using a thick needle and withdrawing the stylet after having perforated the dura mater; I have seen a surgeon of world-wide reputation repeat the puncture 15 times with the thick Quincke stylet before obtaining a little liquid tainted with blood. Is it surprising that under such conditions paraplegia should not have been observed more frequently? So far as I know it has never been observed in wards where spinal anæsthesia is a routine method, because the surgeons who are adepts in spinal anæsthesia are as a rule cautious and exact and avoid these rough manœuvres.

The injection into the cerebro-spinal fluid of irritant substances, such as caffeine, which was advocated a few years ago by Bloch, has caused accidents characterized by the relaxation or irritation of the sphincters, which discredited that dangerous innovation.

On the basis of our personal experience, and of that of all those who regularly use spinal anæsthesia, we can assert positively that the careful injection into the cerebro-spinal fluid of a substance which is little irritant, never causes symptoms of paralysis. The paralysis of the external ocular motor nerve has entirely disappeared.

As to *cephalalgia*, it *has become an exceedingly rare exception*, whereas up to 1928 we had 5 per cent. slight headaches and 1 per cent. headaches of fairly long duration. The question of cephalalgia deserves to be specially commented on.

There is a cephalalgia with hyperpressure of the cerebro-spinal fluid which might be considered the result of a meningeal reaction, and a cephalalgia with hyperpressure of the cerebro-spinal fluid which is much more frequent, and, according to Leriche, results from the persistent oozing of the liquid through the puncture-hole in the dura mater. This ætiology is based on well-established facts, and mainly on the fact that a large number of patients who suffer from cephalalgia with hypopressure are relieved by intravenous injections of distilled water, which, according to Weed and MacKibben, help to restore the tension by stimulating the increase of the cerebro-spinal fluid by the choroid plexuses; it is also based on the fact that headaches are more frequent when the puncture is made with a thick needle.

I do not believe that these hypotheses are correct, and I consider that one should divide headaches into two groups each having a distinct pathogenesis:

(1) Cephalalgia due to meningeal reaction. The simple spinal puncture, and *a fortiori* the puncture followed by the injection of any kind of liquid is liable to cause a meningeal reaction in consequence of the presence of blood within the liquid or of the introduction of cutaneous cells impregnated with iodine or of the infection of the liquid. Every men-

ingeal reaction is accompanied by hyperpression of the liquid. The above described ætiology is indisputable in certain cases of cephalalgia. These are true meningites, from which fortunately the patient recovers in most cases by his inherent resisting power. If the puncture is made skilfully with a very thin needle avoiding repeated puncture, and the asepsis of the syringes is improved through sterilisation at 150°F., these headaches will rarely occur.

(2) Cephalalgia due to cerebral congestion. I no longer believe that this variety of cephalalgia is due first of all to the oozing of the liquid through the puncture-hole in the dura mater. *Hypopressure of the liquid may cerainly result from the sudden withdrawal of a large volume of liquid through the spinal puncture and it is probably* for that reason that cephalalgia is comparatively more frequent when a spinal puncture is made for diagnostic purposes than after spinal anæsthesia. If instead of a spinal puncture a suboccipital puncture is made, cephalalgia is practically never noticed. This is due to the fact that the patient being in a sitting position, the liquid in the lumbar region has a considerable pressure, whereas in the suboccipital region, the pressure is null or sometimes negative. The sudden withdrawal of 10 c.c. of cerebro-spinal fluid in the lumbar region suddenly lowers the column of cerebro-spinal fluid and as a result the medulla oblongata stops up the occipital foramen.

It is for that reason that in the course of a laminectomy carried out in the sitting position the flowing out of the liquid is always very minute, whereas if that operation is performed in the horizontal position the liquid of the cranium oozes out throughout the course of the operation. On the other hand the withdrawal of 10 c.c. cerebro-spinal fluid through the suboccipital puncture does not cause the medulla oblongata to stop up the occipital foramen, because in that region where the pressure is null or negative and where the close proximity of the bulb causes the surgeon anxiety, the liquid is never withdrawn suddenly, but with great gentleness, which enables the liquid of the cranium gradually to replace the liquid withdrawn from the rachis.

The above hypothesis would explain that the methods of spinal anæsthesia which require the sudden and final withdrawal of cerebro-spinal fluid, predispose the patient to post-operative cephalalgia which constitutes a sufficient reason to give up the method. We shall go further and say that whereas the sudden withdrawal of a large volume of cerebro-spinal fluid is sufficient to explain the headache which follows a simple spinal puncture, the injection of a foreign substance, such as we practise is in the course of our spinal anæsthesia liable to constitute a causative factor in cephalalgia. If one carries out, as we do, the progressive and fractional injection, leaving the needle in the spinal canal during a period of time of variable length, one notices, in subjects who are not too rachi-resistant, that the injection of 5 c.c. of percaine solution 1:1500 far from increasing the pressure of the cerebro-spinal fluid causes it to diminish more or less instantly; and as one gradually injects further quantities of anæsthetic solution, the anæsthesia progresses and the fall of the pressure of the cerebro-spinal fluid increases. If the subject is very rachi-sensitive, or if a more concentrated anæsthetic solution is used, the pressure of the liquid becomes finally negative and one sees the liquid re-enter the needle.

If that fall of the pressure (which is no doubt due to modification of the blood circulation within the epidural spaces) is really important, it must necessarily cause the same effects as the sudden withdrawal of liquid during the spinal puncture. In other words the medulla oblongata must stop up the occipital hole.

If this is so, it is clear that the *methods of spinal anæsthesia which require the injection in the sitting position of a very concentrated anæsthetic solution, predisposes the patient in a greater degree to cephalalgia.*

It must be borne in mind that *when the pressure of the cerebro-spinal fluid diminishes, the choroid plexuses secrete a reflex process an increased quantity of liquid to compensate for the fall of the pressure.* Now as the medulla oblongata stops up the occipital foramen, the liquid cannot penetrate from the cranium into the perimedullar space and it increases the intracranial tension.

If one admits the above hypothesis one understands that the Trendelenburg position which diminishes the pressure of the brain on the occipital foreamen and that intravenous injection of distilled water, which diminishes the secretion of the choroid plexuses, can, in certain cases, relieve the patient.

The method which we use combines all the conditions which tend to avoid cephalalgia. We do not withdraw, but on the contrary inject liquid. We do not use a concentrated anæsthetic solution, but on the contrary a very diluted one. Our patients are always placed head down during the injection and after the operation.

Does that mean that cephalalgia has as a consequence become impossible? Such a conclusion would certainly be exaggerated.

Percaine, a quinoline derivation, is certainly a slightly irritant product; the menstruum a 0.5 per cent. solution of sodium chloride in distilled water is not isotonic. Thus Jones's solution must certainly cause a certain meningeal reaction, which possibly explains the rachialgia which is fairly frequent during the first days following the operation.

On the other hand the hypotension of the blood, a phenomenon which is practically constant and also manifests itself within the cerebral circulation, may be followed, in subjects with vascular hypertonia, by a reaction which finds its expression in intracranial hypertension with subsequent cephalalgia or even manifestations of cerebral congestion.

It is with a view to avoiding this cerebral congestion, which may be dangerous with aged patients, that we endeavour with them to prevent a too pronounced circulatory repression by reducing the anæsthesia to the indispensable minimum and by giving ephedrine beforehand.

I am even convinced that spinal anæsthesia normally always causes a slight degree of spinal fluid reaction (reaction liquidienne) and of meningeal reaction with concomitant temporary trouble of the cerebro-spinal circulatory equilibrium as well as of the functions of the liver, of the kidneys and endocrine glands.

No method of anæsthesia leaves the vegetative functions entirely unaffected, but in practice spinal anæsthesia as we perform it, causes so remarkably benign clinical sequelæ that the uninitiated would not notice the slight defects which are its unavoidable consequences.

In practice one can say that cephalalgia has disappeared.

If I have dealt at some length with the question of cephalalgia it is with a view to throwing light on the complexity of the apparently very simple phenomena, which accompany spinal anæsthesia.

Those who, like ourselves, have followed the evolution of this method of anæsthesia and who have witnessed a great many times disquieting manifestations which appeared at the beginning to be entirely unexplainable, always hoped that the physiologists would throw light on the subject through investigating spinal anæsthesia on animals. They have had to wait 30 years. Spinal anæsthesia, which was invented by Corning in 1894, was applied in surgery by Bier in 1898, and in 1928 Leriche could still say that spinal anæsthesia had not yet the good fortune of interesting the physiologists. Even to-day in spite of the enormous number of spinal anæsthesias which are performed every day in the whole world there exist, so far as I know, but half a dozen important papers written by physiologists, and the work presented this year by Derom to the Academy of Medicine of Belgium for the Alvarenga prize is the only one which deals with the phenomena of spinal anæsthesia in animals under conditions fairly similar to those which are realized by the surgeons.

The (so to say) entire safety resulting from our technique diminshes, it is true, to a certain extent the considerable interest which such investigations offered at the time more complicated techniques were used.

The mechanism of spinal anæsthesia and the particularly prominent concomitant phenomena may be summed up as follows:

When one injects into the spinal fluid a solution of normal concentration of any of the anæsthetic substances which have been advocated for spinal anæsthesia one does

not obtain, as was formerly believed, a physiological section of the medulla. When one injects within the spinal fluid a coloured substance, the said substance only faintly tinges the external surface of the medulla, but it fixes itself on the roots which issue from the medulla and impregnates them deeply. The anæsthtetic substance behaves in an identical manner.

This phenomenon is called elective neuropexia; the anæsthetic substance deeply impregnates the nervous fibres of the roots, thus causing physico-chemical modifications which provoke the temporary physiological section of these fibres, which no longer transmit the nervous influx, the roots being temporarily blocked. Spinal anæsthesia is consequently not a medullar but a radicular anæsthesia.

From the medulla emerge on the one hand the posterior sensitive roots which transmit towards the neuraxon the sensations collected at the periphery, and on the other hand the anterior motor roots which transmit the motor impulses from the neuraxon to the periphery.

But the anterior roots contain in addition orthosympathetic fibres which are specially important for the equilibrium of the functions of the vegetative life. Their action is antagonistic to that of the parasympathetic, whose main tract does not pass through the medulla, but runs from the bulb through the neck and the thorax under the name of pneumogastric.

The most apparent functions of the orthosympathetic fibres which issue from the medulla through the anterior roots are the vasoconstriction, the acceleration of the heart, the intestinal relaxation and the closing of the sphincters.

All these functions are probably subordinated to adrenalin, and it appears more and more certain that the adrenalin secretion in the suprarenal glands is stimulated by the orthosympathetic nerves. Schotte of Ghent has contributed an interesting paper demonstrating these facts.

The physiological section of the anterior roots caused by their impregnation through the anæsthetic agent thus does not only suppress the motility, but causes the interruption of the functions of the orthosympathetic nerve, and as the parasympathetic nerve which does not pass through the

rachis, is influenced to a lesser degree and in a more retarded manner by the anæsthetic agent, there is rupture of the equilibrium between the two antagonistic systems and the parasympathetic nerve alone continues to function. This results in vasodilation, slowing of the heart, intestinal contraction, opening of the sphincters, to mention only the most interesting functions related to our subject. As the interruption of the orthosympathetic nerve also interrupts the adrenalin secretion in the suprarenal glands, all these phenomena can develop in a progressive manner as the adrenalin content of the blood gradually diminishes.

This results clinically in hypotension slowing of the pulse, exaggeration of peristalsis, with defecation if the intestines are full, vomiting, etc.

It is remarkable that the anæsthetic agent does not interrupt these fibres with the same rapidity, the same intensity, or the same length of time.

Under the influence of spinal anæsthesia the sensibility to pain disappears first, then the sensibility to heat, then the tactile sensibility, and finally the deep sensibility. This is followed by the suppression of motility and at a later stage only are the functions of the orthosympathetic nerve interrupted.

The return to the normal condition takes place in the reverse order, i.e. the orthosympathetic nerve resumes first its function, then the motor fibres allow the nervous influx to pass, then the sensitive fibres become again conductive, the patient feels the contact, then the heat, and finally the pain. It is certainly a great advantage that the interruption of the functions of the orthosympathetic nerve, which sometimes causes alarming symptoms, should generally be of comparatively short duration, but it is much less pleasant for the surgeon that the blockage of the motility fibres ceases before the end of the operation when the analgesia is still satisfactory.

Fortunately the choice of the anæsthetic agent permits of regulating (so to say) at will, the intensity of the blockage of these three groups of radicular fibres.

Every anæsthetic agent used in spinal anæsthesia exerts a different action on the sensitive motor and orthosympa-

thetic nerves. On the basis of our clinical experiments we may say:

(1) That cocaine gives a good analgesia, a moderate muscular paralysis and that it excites the orthosympathetic nerve instead of paralysing it; if it had no other drawbacks, cocaine, which is a good analgestic agent, would be the ideal anæsthetic for thoracic surgery, because it does not paralyse the orthosympathetic nerve, but it is a poor anæsthetic agent in abdominal surgery, because it paralyses the walls insufficiently.

(2) Novocaine causes an impregnation which is insufficient from the point of view of duration; it is a mediocre analgesic and paralysing agent, whose action on the orthosympathetic nerve is fairly potent.

(3) Stovaine, tropacocaine, and above all tutocaine, cause an impregnation of longer duration. Tutocaine in particular is an excellent analgesic agent; its paralysing action is somewhat more pronounced, but its duration is too short. It affects the orthosympathetic nerve, but moderately.

(4) Percaine, a quinoline derivative, has a very strong impregnating power; it is a real "mordant". It has the same qualities as tutocaine as regards its action on the sensitive and orthosympathetic fibres with the advantage of an appreciably longer duration, but *the factor which characterises it and gives it an entirely special place in the series of anæsthetic agents is the intensity and duration of the blockage of the motor fibres.*

As a result, percaine causes a perfect and prolonger relaxation of the abdominal walls, whereby it has gained the favour of surgeons. As a consequence percaine, if employed without taking into account the rules established by those who have had experience of it, may also cause paralysis of all the respiratory muscles (intercostal and diaphragm). Percaine has been said to be toxic. In spinal anæsthesia, however, the toxicity of percaine is of no importance. The dose employed is infinitesimal if compared with the doses used in local anæsthesia; furthermore, contrary to the opinion expressed by certain surgeons, the absorption of a product injected into the spinal fluid is very

slow. The injection into a vein of 20 c.c. of percaine solution 1 : 1500 without adrenalin causes no disquieting symptoms whatever.

If, however, one injects the same quantity into the spinal fluid of the same subject at the level of the fifth dorsal spine, this will cause alarming manifestations which a surgeon will not easily forget.

The danger of spinal anæsthesia does not result from the toxicity of the product injected, but primarily from the blockage of the orthosympathetic nerve and from the paralysis of the respiratory muscles with all their physiological consequences.

In spinal anæsthesia without percaine the blockage of the orthosympathetic nerve constitutes the main danger. In spinal anæsthesia with percaine the main danger is the paralysis of the respiratory muscles, whereas the blockage of the orthosympathetic nerve plays a less important part.

Our technique, which consists of injecting fractionally at a low level a diluted solution which is made to impregnate first the posterior roots, is the method which at the present stage offers the best means of avoiding too intensive a blockage of the anterior roots of the thoracic and cervical medulla with the dangerous consequences which may follow.

Let us now say a few words on the mechanism of the accidents concomitant to spinal anæsthesia. There exist on that subject many erroneous ideas, and it is indispensable to understand exactly the undesirable phenomena which may manifest themselves, in order to avoid or to combat them.

The theory of the primary bulbar accident must be resolutely abandoned. It was formerly believed, and many surgeons still believe to-day, that the syncope and stoppage of the respiration in the course of spinal anæsthesia resulted from the impregnation of the vital centres of the bulb by the anæsthetic agent. This opinion is erroneous, as shown by the fact that in animals it is possible to deposit on the bulb a strong anæsthetic solution without causing any circulatory or respiratory trouble. These, on the contrary, manifest themselves if the anæsthetic agent is brought into contact with the upper thoracic and cervical roots. The

anæsthetic agent cannot impregnate deeply either the bulb or the medulla.

Does this mean that the theory of the bulbar accident must be abandoned? Not at all; *the serious accident is always caused by a trouble or stoppage of the bulbar functions,* but as mentioned before, this bulbar trouble is never primary, but always secondary to other troubles which are caused by the action of the anæsthetic agent within the spinal canal on the roots which emerge from the medulla.

These secondary bulbar troubles, which are always serious and sometimes even disastrous, occur when the bulb no longer receives the quantity or quality of blood that is necessary to its function, and this is due to the hypotension and anoxemia which in turn result from the blockage of the roots and first of all from the blockage of the anterior thoracic and cervical roots.

It is quite possible that the sudden withdrawal of a large volume of cerebro-spinal fluid from a patient in the sitting position, whose endocrineal tension is high, may cause a primary bulbar accident as a consequence of the compression of the elongated medulla in the occipital foramen, but this procedure, which may have been employed in the initial stage of spinal anæsthesia, is, so far as I know, no longer used in any of the techniques employed to-day.

I believe that we may state that the bulbar accident (collapsus, or respiratory stoppage) is never primary, but always subsequent to quantitative or qualitative troubles of the blood circulation in the bulb; these troubles are caused by the blockage of the roots, and as the blockage reaches higher roots, the danger becomes greater. This is very important because if the bulbar accident is never primary, we shall always be warned of its appearance by premonitory symptoms.

It must be remembered that there are individual differences in the sensitivity of patients to the action of an anæsthetic agent injected into the spinal fluid, but generally speaking, it may be said that the anæsthetic agent, when it reaches the upper thoracic or cervical roots in sufficiently high concentration, causes a marked disturbance of the essential vital functions, i.e., the respiration and circulation.

By blocking the motor fibres in the anterior roots of the thoracic region the anæsthetic agent paralyses the intercostal muscles, while in blocking the anterior cervical roots it paralyses the phrenic nerve and consequently the diaphragm. As a result the patient has no longer any means of introducing air into his lungs. Such a condition, specially with percaine, is not necessarily accompanied by a stoppage of the circulation, and it is quite possible that the intelligence remains to a certain extent lucid, but the patient can neither respire nor talk. He becomes cyanotic, anoxemic, and if one does not come to his help by instituting artificial respiration, or still better oxygen insufflations into his lungs, a secondary bulbar accident may soon take place, which may be fatal.

In such cases the patient should not be made to inhale carbonic acid, as has been suggested, seeing that the patient's blood already contains an excess of it. In order to avoid the bulbar accident oxygen must be given into the lungs, and ephedrine or even adrenalin into a vein; when the greatest danger has disappeared bulbar stimulants, such as carbonic acid, lobeline, etc., may be given, but never at the first stage of treatment.

If the blockage is not so serious—for instance, if the blockage of the anterior roots leaves the cervical roots intact and does not affect the phrenic fibres which supply the nerves to the diaphragm—the respiration is maintained exclusively by the diaphragm (the intercostal nerves being paralysed), which is sufficient to maintain life.

This is a condition frequently seen when one uses percaine, which, as we have said, seems to have a special affinity for the motor fibres.

In such cases the patient exhibits that slight degree of cyanosis which is so characteristic of percaine anæsthesia. It is generally necessary to watch the patient because the diminution of the amplitude of the respiratory movements is a causal factor of circulatory hypotension.

In the normal human organism, as a matter of fact, every deep movement of inspiration causes a vacuum in the thorax; this vacuum does not serve solely to fill the lungs with air, but in addition causes the blood to rise into the *vena*

cava in order to ensure the filling up of the heart. The physiologists have taught us that the filling of the heart is a stimulant that is necessary to start the cardiac contractions. The suppression of the inspiratory movements of the thorax immediately causes the stoppage of the aspiration of the venous blood and the action of the heart weakness. This is how the diminution of the respiratory amplitude is a course of hypotension.

Therefore, when a patient respires badly it is useful to place him in a very slight Trendelenburg position, not only in order to improve the circulation within the brain, but also to ensure a better filling up of the heart.

This is exactly contrary to what was done in the earlier days of spinal anæsthesia. The anæsthetic agent which blocks the anterior roots of a high segment of the medulla puts out of action not only the motor fibres, but also those of the vasoconstrictor orthosympathetic nerve which accelerate the heart and promote the adrenalin secretion.

It is easy to understand that contrary to the law of Marey, according to which in cases of circulatory hypotension the heart's movements are accelerated, we witness here both slowing down of the pulse and hypotension. This has one advantage, i.e., that the wound bleeds less, but it has also one danger, namely the bulbar anæmia, which may cause a secondary circulatory accident and the anæmia of the myocardium, which diminishes the cardiac activity. An anæsthetic agent which blocks the orthosympathetic nerve in the high region of the rachis for a long period is very dangerous for the patient, and if the surgeon is compelled to use that method of high spinal anæsthesia, the patient must be watched very closely, the more so as adrenalin secretion being stopped, the blood becomes rapidly poorer in adrenalin, and as a consequence the hypotension becomes still more severe.

It is thus always advisable, when performing a long operation requiring a high anæsthesia, to give the patient normal saline containing adrenalin before the end of the operation. This becomes absolutely indispensable if the patient is in a weak condition or if the operation has caused severe shock or abundant hæmorrhage, because in such

cases a delayed accident of circulatory origin may prove fatal.

It is because of not having taken into account the few above-mentioned rules that certain surgeons have had to deplore severe accidents after the end of the operation. All this may seem rather disturbing, but in practice there are no accidents. It is always possible to save the patient by injecting adrenalin intravenously and to continue the administration of small doses up to the end of the anæsthesia.

GENERAL INTRAVENOUS ANÆSTHESIA WITH EVIPAN-SODIUM

By Dr. Pasquale Serocca, Naples
(Translated by George Edwards, M.R.C.S., L.R.C.P.)

*(Continued)**

THE muscular tone is not affected characteristically. Often. in fact, while the skeletal muscles seem to be contracted, those of the abdominal wall are relaxed, and vice versa. The faucial and corneal reflexes are abolished with small doses. The pupils are moderately dilated. On the other hand, according to Klages they ought to be contracted and their reflexes almost preserved. De Gironcoli, for example, has never seen them disappear. The visible mucous membranes usually preserve their pink colour: however, a slight pallor towards the end of narcosis has been observed by Klages.

Locally, evipan-sodium is well tolerated. It does not damage the veins, which remain patent even after many punctures for repeated administrations. Nor has any damage been caused by an accidental injection into the perivenous tissues, where the solution remains inactive. Lauber avoids repeated injections, whereas Rode, Sebening and Beck have never met with signs of cumulative action. Rode, in fact, has given evipan-sodium three times in two days in a case of bilateral mastoidites accompanied by high fever. He observed no ill effect. Sebening and Beck have used it nine times in succession to carry out a painful treatment in a girl of 20 and they observed neither intolerance nor venous thrombosis. With regard to this last possibility, Kaspar reports a number of cases following the use of concentrated solutions.

Other biochemical investigations have been made with regard to the liver function, the concentration of sugar in the blood, and the constitution of the urine and fæces up to seven-

* *See* Vol. xi, No. 4, 1934, pp. 153–6.

ty-two hours after the operation (Jarman and Abel). Recognizing the well-deserved importance (in surgery) accorded during recent years to the alkaline reserve, it is necessary to be able to state that the alterations in this factor produced by evipan-sodium are negligible (Monod). We know that ether and chloroform produce a definite fall in the alkaline reserve, and, to find an agent similarly inactive to evipan-sodium in this respect, we must turn to local anæsthetics or to avertin, since spinal anæsthesia causes some diminution (Badel). We wish, whilst dealing with this aspect, to draw attention to the fact that De Gironcoli performed a cholecystgastrostomy under evipan-sodium in a patient of 63, extremely hysterical and markedly cachectic from a carcinoma of the head of the pancreas and that she made an excellent recovery. Psychologically evipan-sodium is welcomed by the patients, as all users of it affirm. Lauber, who has used the drug in patients, previously subjected to inhalation anæsthetics as many as fourteen times, states that they were enthusiastic. Auschintz, having had personal experience, says he can confirm this and gives the explanation: there are no unpleasant symptoms on coming round, no stupor, no nausea, no vomiting, and in a short time the patient is quite clear-headed.

The experience of Klages with 84 children, whose ages ranged from 2½ months to 14 years, is particularly noteworthy. This anæsthetist states that he has never observed any ill effects; a fact of the greatest importance when we consider the pallor and hyperpyrexia which are so often seen following general anæsthesia in children and on which the psychical trauma are said to have so much bearing.

With regard to practical administration Klages finds that immediately after the injection of evipan-sodium there is a relaxation of the muscles of the cheek and of the tongue; the latter organ may fall back with the lower jaw. Moreover, according to Jarman and Abel there is at the beginning of the injection and for some time afterwards, a lessening in activity of the respiratory centre. This must be remembered in dealing with old and debilitated patients. The only two cases which gave trouble to these authors suffered from respiratory failure.

Technique and Dosage.

As has already been said, evipan-sodium is supplied in the form of a concentrated solution (33%) or as a crystalline salt. The latter is preferable. The powdery contents of a phial are dissolved in 10 c.c. of distilled water. Warming is unnecessary. The injection is made into a vein at the elbow. The first four c.c. are injected in a period of one minute so that each c.c. takes fifteen seconds. The patient immediately becomes drowsy and yawns. The rest of the dose can be given more rapidly, one c.c. in every ten seconds. Some anæsthetists prefer to have a pause of fifteen to twenty seconds between the two parts of the injection.

Sebening and Beck describe a fractional method. According to them, the quantity necessary to produce sleep is injected, the needle is left in the vein, and further small quantities are given as need arises. To avoid coagulation of blood in the needle, Bürkle de la Camp and Kirschner have suggested a two-way canula so as to run saline through continuously. By this method Jarman and Abel have been able to exrcise a tibial neuroma keeping the patient anæsthetized for one hour and a half. Lordmana, taking 250 cases in which 10 c.c. was injected, came to the conclusion that it was necessary not to inject too slowly. Klages, in fact, reports four cases in which he had to wait five minutes and another case which, even after 8·5 c.c., did not go to sleep at all; he attributes this to excessive slowness of injection.

The determination of the dose suitable for each case rests with the anæsthetist in charge. The manufacturers of the drug (I. G. Farbe Industrie) provide a scale of dosage according to weight which it is well to follow until one has acquired sufficient experience to judge for oneself. Nevertheless, Rode has used 10 c.c. in every case without regard to weight and has had no trouble. Specht has compiled another table in which he takes into account the weight, age and sex of the patient. Whichever method is adopted it appears that there is no exact relationship between any of these factors and the length of the anæthesia. From the differing views of various anæsthetists it must be inferred that there is a varying reaction to the drug.

Gourbandt and Lauber advise telling the patient to count aloud and observing the amount injected at the moment that the counting stops. This amount should be taken as from one-third to one-half of the total dose required, according to the length of anæsthesia demanded. Klages and Bancks think that from 6 to 10 c.c. should be injected according to the nature of the operation and the general condition of the patient. We wish to add, however, that Jarman and Abel injected 4 cgr. of pantopon and 0·4 mgr. of scopolamine one hour before the operation. Similarly Baetzner gives an injection of morphine-atropine three-quarters of an hour before the operation to facilitate the induction and as a preparation for subsequent ether anæsthesia. This practice, however, is followed by few, being considered superfluous (De Girancoli, Sussi).

Clinical Uses

Evipan-sodium as a means of basal anæsthesia is still *sub judice* in the view of many observers. Whilst on the one hand some anæsthetists have used it successfully for this purpose (Klages, Simenauer and Thieson, Monod), others do not consider it suitable. In fact, Abel prefers pantopon-scopolamine, whilst Lauber, Sebening and Beck, Ernst and others prefer to use avertin. On the other hand, there is complete agreement as to its value as an agent for general anæsthesia for minor operations, intermediate surgery and for emergency work.

More particularly the most suitable cases for its use are the opening of whitlows and abscesses, biopsies, reductions of dislocations and fractures, and also such larger operations as tonsillectomy, hæmorrhoidectomy, surgical treatment of fistulæ and empyemata, amputations of breasts and of limbs, and painful cystoscopic examinations. Jarman and Abel have also used it for œsopagoscopies and bronchoscopies, in various operations on the urinary tract (ureteric catheterisation, diathermy of the bladder), in acute and chronic appendicitis and in gynæcological operations (dilation and curettage with insertion of radium). In a case of bilateral inguinal hernia these observers, nevertheless, had need of a second injection. Rode records nine laparotomies for tubal preg-

nancy and for placenta prævia; in these cases he had sometimes had to use from 80 to 100 grammes of ether in addition. But one of these cases had myocardial weakness, and 7 c.c. of evipan-sodium was enough to allow the completion of the operation.

Weese, following his initial experiments, has recorded 10,000 administrations without death or cause of alarm directly due to the anæsthetic. The uncertainty of the cause of death in a case of curetting for septic abortion caused him to lessen the initial dose to a half (5 c.c.).

In the Second Obstetric and Gynæcological Clinic at Monaco, as reported by Köster, almost all the operations were performed under evipan-sodium without inconvenience. Monod maintains that evipan-sodium can be used for general anæsthesia for short operations or as a basal narcotic with subsequent ether or for short anæsthesia in minor surgery, when he considers it ideal, having used it extensively. Monod's researches reveal that paracentesis, treatment of thoracic fistulæ, drainage of empyemata and the scraping of osteitic cavities have provided a large field for experiment and have shown no ill effects.

(To be Concluded.)

THE DOSAGE OF "AVERTIN" AS A SURGICAL ANÆSTHETIC

By W. R. N. Morton, M.D.

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"AVERTIN FLUID," or tribromethyl alcohol dissolved in amylene hydrate, is generally regarded as a *basal* anæsthetic and is given in doses of 0.1 c.c. per Kg. body weight irrespective of the total weight; the dose being modified, if considered necessary, by the type and clinical condition of the patient. The method is not without danger, as death has occurred under avertin alone when given in this dosage.[1] The supplementary anæsthetics generally employed are nitrous oxide and oxygen, ether, chloroform, or local. The additional volatile anæsthetic has a certain risk of its own; cyanosis and struggling may occur with gas and oxygen and the damage done by anoxæmia can be severe.[2] Ether has been found to be the most frequent cause of respiratory complications with avertin as the basal anæsthetic.[1] Chlorform has a deleterious action on the liver, and as avertin is detoxicated by this organ the damage done may hinder detoxication.

Complete surgical anæsthesia without ill effects has been reported in 25–30 per cent of cases given avertin alone in doses of 0.1 c.c. per Kg.[3, 4, 5, 6] We may conclude that avertin anæsthesia is not necessarily injurious to the patient, and that the depth obtained by this method is not uniform. Various other methods of estimating the dosage of avertin have been proposed, that of Domanig as modified by Anschütz, Specht, and Sebening[7] being apparently an excellent one; although Meyer Saklad's method of fractional dosage leaves less chance of error in assessing the susceptibility of the patient, and resembles inhalation anæsthesia in making dosage depend on observed response.[8]

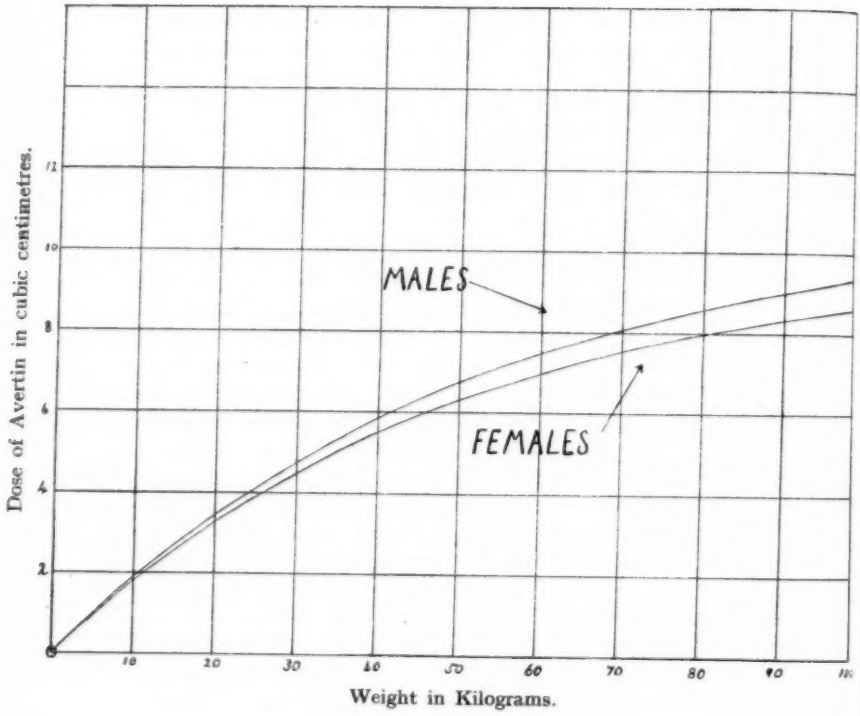
Complete avertin anæsthesia has not been favourably

received, on account of the early reports of Eichholtz[9] and of several fatalities attending its use by other workers. However, there are obvious advantages, and if complete anæsthesia can safely be obtained in 30 per cent of cases with a uniform dosage of 0.1 c.c. per Kg. it is reasonable to hope that this percentage can be increased. Flörken and Meus[10] give 0.13 c.c., or more, per Kg., and find that satisfactory anæsthesia occurs in about 80 per cent of cases. Leder[11] gives 0.125 c.c. per Kg., on an average, for complete anæsthesia, while Boyd[12] gives up to 0.175 c.c. per Kg. to children.

Kirk has been endeavouring with varying success since 1930 to use avertin as a complete anæsthetic, and the present investigation was begun in 1932 in order to get a simple scheme which would be a fair guide to dosage for the normal patient. Records were kept of age, sex, height, sitting height (from chair to top of head), weight, type of operation, dose, and result; and the relationship of these factors was assessed. It was found that dosage could most conveniently be based on the weight alone, and that light people required more avertin per Kg. than heavy people. A tall patient for his weight appeared to be slightly less susceptible than a person of medium height and the same weight. In spite of the fact that avertin is more rapidly detoxicated in hyperthyroidism or after giving thyroxin,[13] the basal metabolic requirements as calculated from age, sex, height, and weight by the methods of Du Bois,[14] or Harris and Benedict,[15] could not be used as a guide to dosage for the average patient.

The results of trial-and-error in attempts at complete avertin anæsthesia were observed over a fairly wide range of weight. It was noticed that if the range of weight were divided into equal intervals of 10 kilograms the increments in dose for successive weight intervals became progressively smaller and formed the terms of a geometric progression. The increments decrease in proportion to their values at the beginning of their respective intervals. The dose at the end of each interval is the sum of the terms of the progression to the number of that interval. The sum of a geometric progression can be determined from the formula:

FIG. 1.


Dose of Avertin in cubic centimetres.
12
10
8
6
4
2
MALES
FEMALES
10
20
30
40
50
60
70
80
90
Weight in Kilograms.

$$s = \frac{a(1-r^n)}{1-r},$$ where s = sum, a = 1st term, r = common ratio, and n = number of terms.

Or, since $\frac{a}{1-r}$ is constant,

$$s = K(1-r^n).$$

Arbitrary values of a and r were substituted, and it was found that the formulae

$$\text{Dose} = 11.0 \quad 1-0.8273^{\frac{\text{Weight in Kg.}}{10}} \quad \text{for males,}$$

and,

$$\text{Dose} = 10.0 \quad 1-0.8200^{\frac{\text{Weight in Kg.}}{10}} \quad \text{for females,}$$

gave satisfactory results.

In practice the curves for these formulas are drawn with weights as abscissæ and doses as ordinates. For a normal patient the dose corresponding to his weight lies between the two curves, tending to approach the upper in males and the lower in females. The doses used by Boyd[12] for avertin anæsthesia in children justify the extension of the curves towards the origin; but for weights above 90 Kg. the indicated dosage was considered too high and was usually reduced by 0.5 c.c.

Reference should be made to the sigmoid curves used by Bazett and Erb[16] for barbiturates. A curve of this type has been drawn for avertin. In most of the range of weight it lies close to the curve of the geometric progression for males and it would be equally suitable as a guide to avertin dosage.

The curves are shown in Fig. 1, and the results of using them as a guide for dosage in males are given in Table I, and in females in Table II, as compared with the results before the curves were used.

TABLE I.

Males:

Anæsthesia	Before theoretical curve		From theoretical curve		Total cases	
	No.	%	No.	%	No.	%
Excessive	1	1.64	0	0	1	0.95
Adequate	40	65.57	34	77.28	74	70.48
Just adequate ...	6	9.84	2	4.55	8	7.62
Barely adequate ...	5	8.20	3	6.81	8	7.62
Inadequate	9	14.75	5	11.36	14	13.33
Total	61	100.00	44	100.00	105	100.00

TABLE II.

Females:

Anæsthesia	Before theoretical curve		From theoretical curve		Total cases	
	No.	%	No.	%	No.	%
Excessive	0	0	0	0	0	0
Adequate	30	75.0	35	85.36	65	82.25
Just adequate ...	3	7.5	2	4.88	5	6.17
Barely adequate ...	3	7.5	4	9.76	7	8.64
Inadequate	4	10.0	0	0.00	4	4.94
Total	41	100.00	41	100.00	81	100.00

It will be seen that there has been an improvement in the percentage of adequate doses and a drop in that of inadequate ones for both males and females since this method of estimating dosage was employed. The criteria observed were as follows: adequate anæsthesia, complete immobility or only very slight movements of the patient during operation; just adequate, movements slight and not enough to hinder operation; barely adequate, small amounts of chloroform necessary to allow operation to proceed; inadequate, where chloroform had to be given before operation could be begun, or for any length of time subsequently; excessive, where the patient gave anxiety owing to poor respiration or pulse, or to long post-operative unconsciousness.

The technique for routine operations is as follows: On

the morning before operation a saline purgative is given, and in the evening a soap-and-water enema. Half an hour before operation a hypodermic injection of four minims of a mixture containing 0.2 per cent atropine sulphate and 4 per cent morphine hydrochloride is given to an adult, or three minims to a juvenile (12–20 years). Immediately after this the patient is placed on his right side and given the avertin enema ($2\frac{1}{2}$ per cent solution). The injection takes about three to four minutes. Twenty minutes later the patient is brought from the ward to the theatre, and the operation can usually be commenced half an hour after the initial injection. If additional anæsthesia is required, a small amount of warmed chloroform is given by means of a Shipway's inhaler for as short a time as possible. Chloroform is used because it has an immediate quietening effect on the patient and post-operative pulmonary complications are not liable to be produced. It has been advocated by Watter in a recent article on this subject.[17]

Signs of overdosage with avertin are treated with intravenous injections of coramine and inhalations of CO_2, but they are infrequent if a proper airway is maintained. The curves are to be regarded as guides to dosage and must not be taken as hard and fast limits. The dose must be modified according to the general condition of the patient. A toxic patient is given less than a non-toxic patient, and females and the aged can usually be successfully anæsthetized with less than healthy male adults of the same weight.

Gyllensvard has shown that avertin is taken up rapidly by the fatty tissues and released again later, so that the course of anæsthesia is modified by the amount of fat.[18] It should be noted that the patients dealt with in this series were usually of thin or moderate build and that obese patients were rare. It is possible that the constants of the curves might require some adjustment for districts where stout patients abound.

No direct measure of the risk run by using avertin as a complete anæsthetic was available, so a comparison was made between the gross post-operative mortality during the period in which avertin had generally been used and that for a proceeding period. In the control series of 1,111

cases, 50 patients died within seven weeks of operation, giving a death-rate of 4.49 per cent. In the second series, 1,346 cases out of a total of 1,709 received avertin either as a basal or a complete anæsthetic. Of these 63 died within seven weeks, giving a death-rate of 4.68 per cent. But the gross death-rate for the whole of the second series (when those who had not received avertin are included), is 5.03 per cent. Analysis of the cases showed that the majority of the deaths occurred in patients who were bad surgical risks and who would probably have died whatever anæsthetic was used, but in seven cases avertin appeared to be at least partly to blame. Five of these cases were, however, acutely toxic; another was suffering from chronic intestinal stasis and died from broncho-pneumonia 15 days after operation; and the last was a very obese woman of 60 years who got 9.3 c.c. avertin in basal dose (0.1 c.c. per Kg), and who became cyanotic and died of cardiac failure without regaining consciousness.

Resume of Pharmacological Reactions to Complete Avertin Anæsthesia.

Blood-pressure falls immediately, and sleep comes on rapidly. Venous pressure also falls at first. Operative interference causes a rise in blood-pressure and venous pressure. Man's average fall in blood-pressure is 50mm. Hg. in systolic and 30mm. Hg. in diastolic pressure. False Duroziez-murmur often audible. Electrocardiograms showed only slight changes.

Respirations slowly increased in rate up to 30 to 40 per minute and diminished in depth. Respiratory volume reduced from 6–8 litres per minute to about 3 litres at rest. Operation raised min. volume to 5–6 litres. Breath volume is reduced from 600–700 c.c. to 180 c.c. at rest, and to about 200–250 c.c. when operation is in progress. Ventilation appears to be adequate so long as a clear airway is maintained.

Sugar metabolism. Blood-sugar shows a definite rise 10 minutes after avertin has been given, reaching a maximum in 1½ hours. The fall to normal is slow.

Renal System. The volume of urine is reduced to 300–400 c.c. per for first 24 hours. Occasionally catheterization is necessary. Specific gravity is high. Ketone bodies occur in small amounts and are not appreciably diminished by giving glucose by mouth just prior to operation. The excretion of avertin in the urine was found to take place for at least four days after administration. Using a hitherto unpublished method, from 45–75 per cent of the amount given was recovered from the urine in the first 24 hours, and from 80–90 per cent in the first 48 hours, confirming Parsons'[19] and Welsch's findings.[20]

Summary.

1. A method is given for estimating the dose of avertin for " complete " surgical anæsthesia.
2. The response of the patient to complete avertin anæsthesia is briefly described.

Acknowledgement.

I wish to express my indebtedness to Mr. T. S. Kirk, senior honorary surgeon to the Royal Victoria Hospital, Belfast, for permission to investigate the cases under his care, and to the various other members of the staff for their willing help and assistance.

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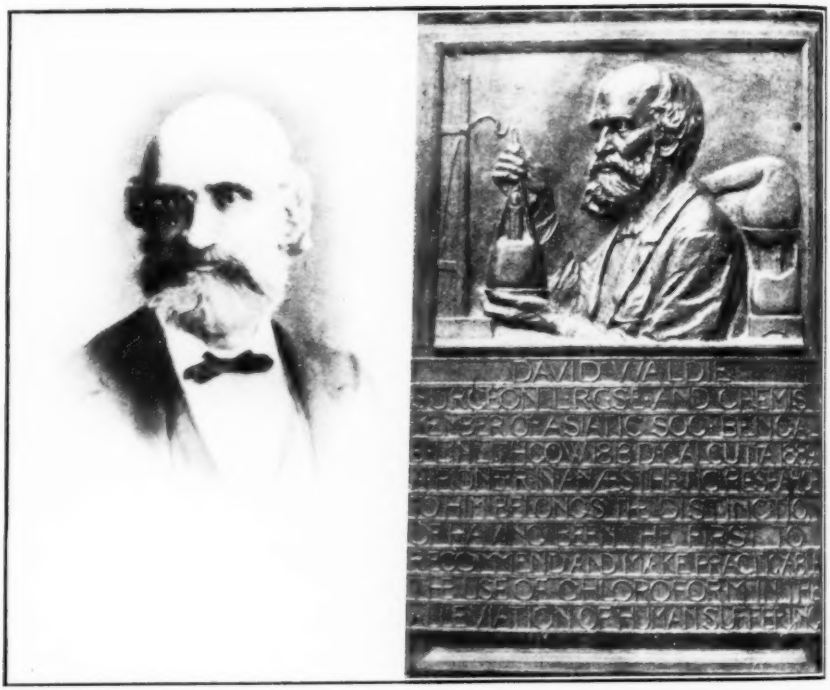


DAVID WALDIE
(L.R.C.S. EDIN.)
Born at Linlithgow, Scotland, 1813
Died at Calcutta, 1889

MEMORIAL AT LINLITHGOW, SCOTLAND

WHO WAS THE PERSON WHO DISCOVERED CHLOROFORM FOR ANÆSTHESIA: WAS IT SIMPSON OR WALDIE?

By A. J. O'Leary, L.R.C.P., etc.
Honorary Consulting Anæsthetist, Liverpool Royal Infirmary.

(Presidential address to Liverpool Society of Anæsthetists).

In my last address I stated that the "Conception of Chloroform took place in Liverpool, but that its birth took place in Edinburgh."

I have now to alter that statement—the conception and birth both took place in our city.

This I do after having a talk with Professor Dilling last February, and before commencing, I have to thank him for the information he gave me about Waldie and also to state that when he has the time he will publish facts about Waldie.

I have also to tender my sincere thanks to Mr. A. C. Abraham and Miss E. C. Abraham, the son and daughter of Mr. John Abraham who was a friend of Waldie, for placing at my disposal all papers, documents, photos and information they had in their possession about Waldie for the preparation of this address.

David Waldie was educated in Edinburgh as a surgeon, where he took the Diploma of the Royal College of Surgeons. He practised as a medical man in Linlithgow, Scotland, for a time, but his inclinations were towards chemical studies and research. He succeeded Dr. Brett as a chemist in the laboratories of the Liverpool Apothecaries Company, Colquitt Street, and in a letter from Mr. A. C. Abraham to me last December, he states "that his father, Mr. John Abraham, was head of the dispensing and retail department, and that Mr. Clay was what one would call now the Managing

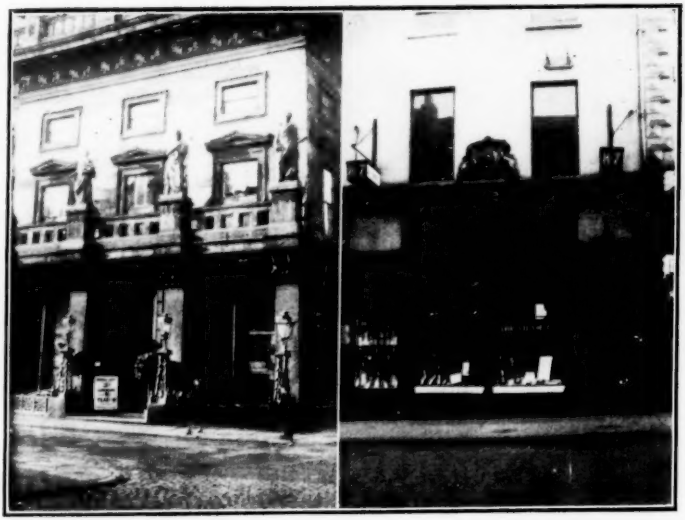
Director. Mr. John Abraham, and Mr. David Waldie, were under him."

Mr. Clay and Mr. John Abraham eventually went into partnership and founded the firm of Clay and Abraham, which became and are still the leading pharmacists in our city.

It was here at the Apothecaries Hall that Waldie first began his experiments on chloric-ether, it having been previously prepared by his predecessor, Dr. Brett, specially for Dr. Richard Formby (who was at that time one of the physicians at the Liverpool Royal Infirmary).

The first thing Waldie did was to purify and make it more uniform, as the chloric-ether before his advent was very crude, and as he was asked by Simpson (who was not satisfied with ether for his work) could he not find something better than it.

He next turned his attention to experiment on the active principle of chloric-ether—namely, chloroform—and while he was doing this the laboratories got destroyed by fire and were not replaced, and according to Professor Dilling, Mr. John Abraham came to his aid and let him continue his experiments at his residence, 87 Bold Street. To prove that this is a fact, in a letter I had from Mr A. C. Abraham he states: "That he did so I feel quite certain from what my father and mother, or both, told me. My mother used to say that she on going into the drawing-room on one occasion was very much alarmed to find Mr. Waldie and my father experimenting on one another with chlorform; one of them, I think, was insensible on the sofa." The same was told to me by Mr. John Abraham's daughter (Miss E. C. Abraham). She stated that her mother told her that she had several times gone into the drawing-room at 87 Bold Street and found this happening. On the first occasion she was alarmed, but, when it happened more than once and they got all right again she took no more notice of it. Mr. Fell Abraham, another of Mr. John Abraham's sons, in a letter of his to the *Daily Post and Mercury* of the 9th of June, 1911, records the same occurrence. So we see by this that David Waldie was fully aware of the action of chlorform before recommending it to Simpson.



The building of the Apothecaries Hall still exists. It was opened in 1836, and is situated at 4 Colquitt Street, Bold Street, Liverpool. Having fallen on evil times it is now rented and used by the Burlington Rooms and Café, Ltd.

Where Waldie began his experiments with chloroform.

CLAY & ABRAHAM
87 Bold Street, Liverpool.

The place where Waldie finished his experiments.

If we refer to the address by David Waldie on the introduction of chloroform into anæsthetics delivered by him at a meeting of the Liverpool Literary and Philosophical Society, held at the Royal Institution, Colquitt Street, on Monday evening, the 29th of November, 1847, we will find on page 8: "Chloroform I recommended to Simpson to try for his work. I sent a copy of my address to Dr. Simpson, but he never alluded to it either by letter or personally."

When David Waldie heard of Simpson's death, he wrote to his brother, George Waldie, a letter entitled "Restatement by David Waldie." This his brother, George Waldie, published and circulated in England in 1870. On page 25 we find David Waldie states: "I have no wish to detract from the fame of Sir James Simpson. I have the highest admiration of his great and rare talents. I never got any greater credit from Dr. Simpson for my being the principal means of obtaining his widespread renown than that was conveyed in a footnote to his original announcement to the Medico-Chirurgical Society of Edinburgh. November 10th, 1847, that Mr. Waldie first mentioned chloroform to him. I did not merely mention it; I distinctly recommended him to try it for his work. His reputation cannot suffer by my getting credit for what I am justly entitled to, and that is all I ask, and I would willingly entertain the hope that had Simpson been still living amongst us, and my claim been placed before him, as it is now before the public, he himself would have admitted its justice."

Therefore, brother anæsthetists, the two words *mention* and *recommend* possess absolutely different meanings. One may mention a thing without knowing a single scrap about it, but when one recommends anything he must be fully alive to what it can do. This is what Waldie did when he recommended chloroform to Simpson for his work, he (Waldie) having previously proved its merits, knowing exactly what the result would be.

So we see that it was Waldie who was the active agent in the discovery, and Simpson only the passive one. The latter got all the honours and was knighted. Waldie, the discoverer, did not even get recognition from one who used to be his friend. *Quod erat demonstrandum.*

Thus I have tried to prove that Waldie was the discoverer of chloroform anæsthesia. He never did anything by halves; it was either the whole or nothing.

I might suggest that until a suitable memorial be erected, a tablet should be affixed by our Society at 87 Bold Street recording the fact that it was here that David Waldie, surgeon and chemist, carried out his research work in the discovery of chloroform anæsthesia.

David Waldie whilst in Liverpool lived at 84 Islington down to 1853; this house still exists, but the lower portion has been converted into a smoke-room for the public-house adjoining. He was of a religious turn of mind, and when in Liverpool published a pamphlet entitled *The Ultimate Manifestation of God to the World*, 1847.

He went out to India in 1853 to a firm there. Afterwards, according to "Notes and Queries" in the Liverpool *Daily Post and Mercury*, 16th June, 1911, he started on his own account as a manufacturing chemist in the Cossipore Chemical Works. In addition to his manufacturing business, he was chemical expert to all the merchants in Calcutta in analysing samples of shellac and other articles of East Indian produce, for which he got a fee. His certificate was accepted throughout Calcutta.

He gave an address before the Asiatic Society of Bengal, November, 1873, "On the muddy water of Hugli during the rainy season with reference to its purification and to the Calcutta water supply."

He died at Cossipore, Calcutta, 25th June, 1889.

A memorial has been erected to him in Calcutta, and also one in his native town, Linlithgow, Scotland, bearing this inscription: "David Waldie, Surgeon, L.R.C.S.E. and Chemist. Born at Linlithgow 1813. Died at Calcutta 1889. A pioneer in anæsthetic research. To him belongs the distinction of having been the first to recommend and make practicable the use of chloroform in the alleviation of human suffering."

And we, the Society of Anæsthetists, ought to see that as there is a Sir William Michel Banks memorial lecture, also a Hugh Owen Thomas one—the latter, alas! only being honoured long after his demise—why should history not

repeat itself and let there be a David Waldie memorial lecture, as in the case of the former two?

Let the propaganda be started and set the ball rolling. There are plenty of Waldie admirers in our City, and we would soon have sufficient funds available for us to ask our Honorary Secretary to interview the Vice-Chancellor of our University, asking him, on our behalf, to institute the said. By such we could pay homage to the memory of one who has remained far too long unhonoured by his adopted City.

In conclusion, I wish to thank Mr. Robert Turnbull for the reproduction of the photographs for this address.

ABSTRACTS

Caudal anæsthesia in urology and proctology. V. F. ONHAUSER in *Canadian Medical Association Journal*, July, 1934, p. 51.

Sacral analgesia is too little employed, the author thinks, for he finds that it gives uniformly successful results. His series of cases is, however, small. The only objection found is time consumption, as he waits half an hour or so after making the injection. The technique is simple and well described. He uses barbital for premedication, giving a dose on the preceding night and one an hour before operation.

In the same journal at page 44 Wesley Bourne contributes an excellent estimate of the usefulness of some of the new anæsthetics. He draws attention especially to the unmerited neglect of ethylene, which deserves much wider employment than it is receiving, at least in Great Britain. Cyclopropane, in Bourne's opinion, is likely to prove of even greater value than ethylene. He has used it with satisfaction in obstetric cases. "Vinethene" (dirinyl ether) he has used for the same purpose, and finds it admirably suited to patients in labour. After briefly reviewing the barbiturates, Wesley Bourne finishes with paragraphs devoted to avertin, of which he says that its revival "has enlarged the bounds of anæsthesia in a most beneficial manner."

Studies in subarachnoid anæsthesia. F. W. COTIN. *Anæsth. and Analg.*, July-August, 1934, pp. 145 *et seq.*

The chief problems at which the author aimed his experiments were (1) the phenomenon of vomiting in spinal anæsthesia, (2) the relation of the position of the animal to the spinal lethal dose, the hydrodynamic laws governing the spread of the injected liquid and blood pressure changes. With regard to vomiting, he arrives decisively at the conclusion that it is due to the action of the anæsthetic on the

vomiting centre, i.e., he disputes the common conceptions that vomiting arises through cerebral anaemia or through mechanical effects on the stomach secondary to respiratory alterations.

Control of pain in obstetrics and gynæcology. SCHMERZ, *Narkose u Anæsth.*, July, 1934, p. 6. C. Margraf.

The author expresses himself as well pleased with a combination of codein-phosphor, phenacetin and acetylsalicylic acid. Unfortunately he gives no detail as to dosage. The effect begins ten to forty-five minutes after introduction of the dose. He calls the compound gelonida antineuralgica.

On page 17 of the same journal E. Schehl discusses the treatment of the vomiting of pregnancy by means of narcosis. He has used a preparation of pernocton by the rectum and another bromide preparation called brosedaw.

Intravenous sodium soneryl as basal narcotic. B. MOPLAS and E. CHEVILLON in *Bull. et Nem. L.C. Soc. Nat. de Cher,* March 31, p. 519.

Sodium soneryl is butyl-ethyl-malonyl-urea, a barbiturate soluble in crystalline form in water. It has been used to a small extent already in Great Britain. The French authors have tested it orally and subsequently employed it by intravenous injection as a basal narcotic in some 87 cases. They have found it most satisfactory, producing but slight acceleration of pulse with some diminution of frequency of respiration, some lowering of blood-pressure and of temperature. The renal and hepatic functions are not interfered with, and recovery is uneventful except that in some instances there was slight restlessness after twelve hours. Unconsciousness interrupted by occasional short periods of wakefulness lasted from 24 to 48 hours after operation. Used in association with ether the amount of the latter needed was much diminished, and the muscular relaxation obtained was admirable.

One thousand avertin anæsthesias. W. BOURNE and P. E. O'SHAUGHNESSY. *Canadian Medical Association Journal.* Sept., 1934. pp 276 *et seq.*

This is, we believe, the largest series of cases of avertin

anæsthesia reported on in English. The analysis of symptoms and results shall go far to reassure any who may still have doubts as to the value and the safety of avertin. It shows clearly that in none of the 25 mortalities following operation in the series could avertin be justly blamed even partially for the death. This occurred from hæmorrhage or from the pre-existing desperate illness of the patient. The authors advocate larger doses of avertin than are commonly employed, believing that these larger doses, supplemented by "gas and oxygen", in which oxygen can be employed in unusually high percentages, are perfectly safe. They point out, however, the necessity for careful examination of the patient when determining the avertin dosage. Mere weight is an insufficient criterion. The general constitution, both bodily and mental, age, and the condition for which the patient is undergoing operation, must be carefully considered. The young and strong, as well as the alcoholic, require larger; the feeble, anæmic and hypothyroid subjects require smaller than the ordinarily prescribed amount of avertin. Careful analysis of post-operative results as regards sickness show a general average of 19.4 per cent. When no other anæsthetic was used the percentage was only 7.4. The authors believe in slow instillation of avertin, at least ten minutes being taken over this. They like oxygen and CO_2 inhalation during the recovery period; they do not mention, and presumably avoid, concurrent use of other preliminary narcotics.